The secretion-cells are subjected to the creative activity of a composite organism, the preservation and regeneration of which they subserve; their task is synthesis. On the contrary it is the office of these ferments to decompose dead and dying organic substances into their simpler compounds, and to prepare them for decay; their task is analysis. To regard these ferment-cells (*Hysterophymata*) as equivalent to perfect organisms shows a complete misconception of their true nature.

Schaffhausen, November 1873.

PROCEEDINGS OF LEARNED SOCIETIES.

ROYAL SOCIETY.

May 8, 1873.—Francis Sibson, M.D., Vice-President, in the Chair.

"Contributions to the Study of the Errant Annelides of the Older Palæozoic Rocks." By H. Alleyne Nicholson, M.D., D.Sc., M.A., Ph.D., F.R.S.E., Professor of Natural History in University College, Toronto.

In this communication the author endeavours to elucidate the abundant and obscure organic remains which are found so commonly in the Palæozoic rocks, and especially in the Silurian strata of Britain, and which are generally known by the vague and convenient names of "Fucoids," "Annelide-burrows," and "tracks." After expressing his opinion that the first step towards the study of these obscure fossils lies in the provisional grouping and naming of the more marked forms which are already known to exist, the author proceeds to divide the remains under consideration into two great groups. In the first of these groups are those fossils which are truly the *burrows* of marine worms, as distinguished from mere trails and surface-tracks. Some of these burrows (*Scolithus*) are more or less nearly vertical in direction as regards the strata in which they are found; and they are to be looked upon as being true burrows of habitation. In this section are placed the genera *Scolithus*, *Arenicolites*, and *Histioderma*. Other burrows are of a totally different nature from the preceding, and may reasonably be compared to the burrows of the recent lobworms. These burrows run more or less horizontally as compared with the laminæ of deposition, or they penetrate the strata obliquely. They are not burrows of habitation, but are wandering tunnels excavated by the worm in its search after food. The fossils of this group, therefore, as preserved to us, are not the actual burrows themselves, but the burrows filled up with the sand or mud which the worm has passed through its alimentary canal. The burrows of this kind (including many forms previously described under the names of *Chondrites*, *Palæophycus*, &c.), the author groups together under the name of *Planolites*.

The second great group of Annelide-remains comprises genuine surface-trails or "tracks," which of necessity never pass below the surface of the bed on which they occur. Some of these remains, such as *Crossopodia*, are, beyond doubt, due to the operation of marine Annelides; but it may be a matter of question whether we have in these cases the actually petrified body of the worm, or merely the track produced by the passage of the animal over the surface of the mud or sand. The author, however, gives reasons for believing that the latter explanation is truly the correct one. Other fossils belonging to this group (such as *Myrianites*) are equally, beyond doubt, produced by the operations of marine animals; but it remains quite uncertain whether they have been formed by Annelides, Crustaceans, or Mollusks. Lastly, there are remains which appear to be really *casts* of the surface-trails of Annelides or other marine creatures, and which, therefore, are elevated above the surface of the bed on which they occur. Such remains may readily be confounded with those belonging to the genus *Planolites*, from which they are only distinguishable by the fact that they are strictly confined to a single surface of deposition. To fossils of this nature the author proposes to restrict the generic title of *Nemertites*.

Finally, the author describes some singular tracks apparently produced by Crustaceans belonging to the genus *Ceratiocaris*, and for which he proposes the generic name of *Caridolites*.

The following list comprises the species of fossils described in this communication:—

A. Burrows.

I. Genus Arenicolites, Salter.
1. Arenicolites sparsus, *Salter*.
2. —— didymus, *Salter*.
3. —— robustus, *Nicholson*.

II. Genus Scolithus, Haldeman.
4. Scolithus canadensis, *Billings*.
5. —— linearis, *Hall*.
6. —— verticalis, *Hall*.

III. Genus Histioderma, Kinahan.
7. Histioderma hibernicum, *Kinahan*.

IV. Genus Planolites, Nicholson.
8. Planolites vulgaris, *Nicholson*.
9. —— granosus, *Nicholson*.
10. —— articulatus, *Nicholson*.

B. Trails.

V. Genus Crossopodia, M'Coy.
11. Crossopodia scotica, *M'Coy*.
12. —— lata, *M'Coy*.

VI. Genus Nemertites, M'Leay.
13. Nemertites Ollivantii, *Murchison*.
14. —— (Palæochorda) major, *M'Coy*.
15. —— (Palæochorda) minor, *M'Coy*.

VII. Genus Myrianites, M'Leay.
16. Myrianites tenuis, *M'Coy*.
17. —— Murchisoni, *Emmons*.

C. Appendix.

VIII. Genus Caridolites, Nicholson.
18. Caridolites Wilsoni, *Nicholson*.

June 19, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

"Experiments on the Development of *Bacteria* in Organic Infusion." By C. C. Pode, M.B., Demonstrator to the Regius Professor of Medicine, and E. Ray Lankester, M.A., Fellow and Lecturer of Exeter College.

The following passage from Dr. Charlton Bastian's 'Beginnings of Life' (vol. i. p. 429) induced us to make experiments similar to those mentioned in it, with the view of testing the correctness of Dr. Bastian's conclusion as to matter of fact:—

"On the other hand, the labours of very many experimenters have now placed it beyond all question of doubt or cavil that living *Bacteria*, *Torulæ*, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F., even for one or two hours. This result is now so easily and surely obtainable, as to make it come within the domain of natural law." And in a note is added, "In a very large number of trials I have never had a single failure when an infusion of turnip has been employed; and from what I have more recently seen of the effects produced by the addition of a very minute fragment of cheese to such an infusion (see Appendix C, pp. xxxiv–xxxviii), I fully believe that in 999 cases out of 1000, if not in every case, a positive result could be obtained." Though this is one out of a great number of statements made by Dr. Bastian upon which he bases speculations as to the prevalence of spontaneous generation or archebiosis, we think it necessary to state that we have not considered that (which is a question of interpretation) as the point at issue, but merely the question of fact as to the appearance of *Bacteria* in what may be considered, according to our present lights, infusions duly guarded from inoculation. The point under discussion is one as to a fact in the natural history of *Bacteria*, in a further study of which we are occupied at the instance of the Radcliffe Trustees; and we believe that a more precise knowledge of the life-history, life-conditions, and various forms of these organisms is necessary before the hypothesis of their spontaneous generation can serve as a safe guide in scientific investigation.

The experiments recorded below were made with infusion of hay and with infusion of turnip, sometimes with the addition of a few fragments of pounded cheese. It is necessary at once to call attention to three precautions which we have taken, and which we think are indispensable:—1. Recognizing the fact that the presence of lumps is a possible source of error, we excluded these from our infusion, either by filtration or by decantation. 2. To ensure the satisfactory exposure of the whole contents of the tube to the boiling temperature, we, as a rule, *completely* submerged our experimental tubes in boiling water for a period varying from five minutes to half an hour. 3. The substances used in preparing the infusions being necessarily of a very heterogeneous nature, we always examined samples of the infusions before and after boiling, at the time of closing the tubes, and were thus able to determine whether any *change* had taken place in the visible particles contained in the fluid after a lapse of time.

The microscopes used by us throughout, working side by side with samples from the same infusion, were a Hartnack's Stative VIII. objective No. 10 à immersion, ocular 4, belonging to the anatomical department of the University Museum, and a large Powell and Lealand belonging to the Radcliffe Trustees, which is provided with a $\frac{1}{12}$ and a $\frac{1}{50}$ objective. The former of the two English glasses was more usually employed than the latter, on account of its greater convenience in manipulation.

Appearances in freshly prepared infusions.—Since the objects seen in such infusions are remarkable, and have doubtless sometimes led to error in subsequent examination of infusions, we may draw attention to them now. In such freshly prepared infusions we have not unfrequently seen appearances agreeing very closely with some of those figured by Dr. Bastian in his book as coming into existence *after* boiling, sealing, and preservation in a warm chamber. A freshly prepared and boiled strong infusion of hay may present shreds of vegetable fibre, a considerable number of dead *Bacterium termo* (some two or three to the field), minute, highly refringent spherules, varying from the size of a blood-corpuscle to the smallest size visible; and such spherules are often present in pairs, forming figure-of-8-shaped bodies, both smaller and larger than *Bacterium* and of different optical character. Further, dumb-bell-shaped bodies are not unfrequently to be observed of similar form and size to *Bacteria*, but coarser in outline; they dissolve on addition of HCl, which *Bacteria* do not*. All these bodies exhibit constant oscillatory (Brownian) movements. The addition of new cheese to such an infusion (as shown by

* In the most carefully guarded of the experiments published by Dr. Child a few years since in the 'Proceedings of the Royal Society,' a very small number of bodies similar to these were obtained; and we suggest that they were of the same nature.

examination of a simple infusion of new cheese taken by itself) adds a considerable number of highly refringent spherules of various sizes (oil-globules) and finely granular flakes, also a few *Bacteria* and (if the cheese be not quite new, almost certainly) fungus-mycelium and conidia in quantity.

Fresh-boiled turnip-infusion alone may contain so very few dead *Bacteria* that none are detected with the microscope, or only one in a drop. It presents a great number of minute, highly refringent spherules, varying in size from $\frac{1}{5000}$ inch downwards, all in most active oscillatory movement. Shreds and filaments of various sizes and character also are found, and a few finely granular flakes about $\frac{1}{1000}$ inch in diameter. The addition of cheese brings in, of course, the objects enumerated above as belonging to it.

Visibility of Bacteria.—It is perhaps necessary to say, before proceeding further, that we have satisfied ourselves that, in infusions of the optical character of those used, the multiplication of *Bacteria* makes itself obvious by a cloudiness. Hence, though we have not remained content with that evidence, the retention by such a limpid infusion of its limpidity is a proof of the absence of *Bacteria*. We also should mention, what is well known already, that in a closed tube or bottle, after such a cloud (of *Bacteria*) has developed, the *Bacteria* at a certain period cease to multiply and settle down as a fine powder, leaving the fluid again clear. Such precipitated *Bacteria* remain unchanged in the fluid for a long period (weeks certainly, perhaps years), and can be readily shaken up and at once recognized by microscopic examination; they are, moreover, not destroyed by boiling: hence it is not possible to miss the detection of a development of *Bacteria* in a limpid turnip-infusion, examined daily for three weeks or more by the naked eye, and finally, after agitation, by means of the microscope.

Series A. Nov. 23rd. *Experiments with hay-infusion.*—An infusion was prepared by pouring water of about 90° C. on to chopped hay. The infusion was of a dark sherry-colour; reaction slightly acid. The glass tubes used in this and subsequent experiments were about five inches in length, of half inch bore, rounded at one end and drawn out to a capillary orifice at the other. The infusion in these and subsequent experiments was introduced by heating the tube and plunging its capillary beak beneath the surface of the experimental liquid during the cooling of the expanded air, until the tube was about one third or half filled. Tubes 1, 2, 3 were half filled with the hay-infusion previously filtered, the liquid was boiled in the tube, and the capillary beak fused, as nearly as possible, during ebullition *.

* The tubes were sealed at the moment of removal from the flame over which they had been boiling. In every case a subsequent recurrence of ebullition was observed during the cooling of the upper part of the tube. Dr. Roberts, of Manchester, has suggested that the occurrence of *Bacteria* in tubes thus sealed may be explained by their in-draught, together with a certain amount of air, at the moment of closure; but the experiments of Sanderson, recently confirmed by Cohn, have shown that contamination of fluids by *Bacteria* only takes place through the medium of impure surfaces or liquids.

Tubes 4, 5 were similarly treated, with the difference that a small quantity of cheese, in a very fine state of division, had been added to this portion of the hay-infusion before its introduction into the tubes.

Tubes 6, 7. Quantity and character of the infusion as in 1, 2, 3, but the tubes sealed without previous ebullition.

Tube 8. Quantity and character of the infusion as in 4 and 5, but the tube sealed without previous ebullition.

Tubes 9, 10, 11. Quantity and character of the infusion as in 1, 2, 3, but rendered slightly alkaline with KHO. Sealed approximately during ebullition.

All these tubes (1 to 11) were after closure completely submerged in boiling water for fifteen minutes, and were then preserved in a hot-air bath, varying in temperature from 30° C. to 35° C.

Microscopic and naked-eye appearances of the hay-infusion at the time of sealing the tubes.—The infusion in tubes 1, 2, 3, 6, 7 was clear and pellucid, that in tubes 4, 5, 8, 9, 10, 11 was hazy.

Microscopic examination gave the result indicated above, as to the appearances of freshly prepared hay and hay-and-cheese infusion.

Subsequent appearances of the infusion in Tubes 1–11.—The tubes with infusion which was pellucid at the first were found to retain this character for several weeks, being preserved in the air-bath and examined from day to day. The hazy infusions were opened after four days, and their contents found to be unchanged.

A portion of the same hay-and-cheese infusion, boiled and purposely contaminated by preservation in an uncleaned beaker, was found after four days to be teeming with *Bacterium termo* exhibiting vital movements. The pellucid infusions were subsequently examined with the microscope at different times and found to be unchanged.

Series B. Nov. 25th. *Experiments with turnip-and-cheese infusion.*—An infusion was made with 700 grms. sliced white turnip and 1000 grms. water, to which about 1 grm. finely minced new cheese was added, the jug containing the mixture being maintained for four hours on a sand-bath at a temperature of 45°–55° C.

The infusion was now filtered; sp. gr. of the infusion 1011·1. Reaction slightly acid.

Tubes 12, 13, 14. Sealed cold. Submerged in boiling water for thirty minutes.

Tubes 15, 16, 17, 18, 19. Sealed approximately during ebullition. Submerged in boiling water for thirty minutes.

The tubes were preserved in the air-bath as in Series A.

Microscopic and naked-eye appearances of the infusion at the time of sealing the tubes.—The liquid in all the tubes was perfectly clear and limpid. A few shreds and flakes were obvious, which appeared to be derived from the filter-paper and from the slight precipitation of albuminous matter. The microscopic appearances were those above described as characterizing such infusions.

Subsequent appearances of the infusion.—The infusion in all the tubes was found on examination from day to day to retain its limpidity. Subsequent microscopic examination of all the tubes at various periods subsequent to the closure of the tubes (from four days to three weeks) yielded no indication whatever of a development of *Bacteria* or other organisms, nor of any change. A portion of the same infusion placed in an uncleansed beaker for comparison was milky and swarming with *Bacteria* after three days.

Series C. Nov. 28th. *Experiments with turnip-and-cheese infusion.*—The infusion similar in all respects to that in series B, but prepared with a somewhat larger proportion of turnips; therefore of higher specific gravity, which was not numerically determined.

Tubes 20, 21, 22, 23. Boiled and sealed approximately during ebullition. Not subsequently submerged.

Tubes 24, 25. Boiled and sealed approximately during ebullition. Subsequently submerged in boiling water during thirty minutes.

The tubes were preserved in the air-bath as in Series A and B.

Series D. Nov. 30th.—An infusion prepared as in Series B and C, but brought to a sp. gr. 1031 by evaporation after filtration.

Tubes 26, 27, 28, 29. Sealed cold. Subsequently submerged in boiling water for thirty minutes.

Tubes 30, 31. Boiled and sealed approximately during ebullition. Not subsequently immersed.

Tubes 32, 33. Boiled and sealed approximately during ebullition. Subsequently submerged in boiling water for thirty minutes.

Appearances in the infusions, Series C and D, at the time of sealing and submerging.—The appearances in the freshly prepared infusion were similar to those described above as characterizing such infusions.

Subsequent naked-eye examination of the tubes did not reveal the slightest change; they remained limpid. Specimens from each group were opened and examined with the microscope after four days, and the microscopic characters found to be unchanged: the liquid was perfectly sweet. The remaining tubes were examined at intervals before the end of December, being maintained during the whole time at a temperature of 35° to 40° C. in the air-bath; they equally proved to have remained unchanged when opened and examined with the microscope, and were also free from unpleasant smell.

Series E. Nov. 28th.—Six porcelain capsules were heated to redness, and nearly filled with the turnip-infusion used in Series C. They were placed on the air-bath under a glass shade.

Capsules 1, 2. The infusion was unboiled.

Capsule 3. The infusion was boiled in the capsule.

Capsule 4. The infusion was introduced after it had been boiled for five minutes in a superheated test-tube.

Capsules 5 and 6. The infusion was that used in capsule 4, but a drop of distilled water was added to each of these two capsules.

After four days the infusion in capsules 1, 2, 5, and 6 was found to be teeming with *Bacterium termo* and Bacterian filaments.

Capsule 3 was found to be cracked, and hence was discarded (it swarmed with *Bacteria*).

Capsule 4 was perfectly free from organisms, and remained so during a fortnight, when a fungus-mycelium made its appearance on the surface.

Series F. Dec. 10th.—A strong infusion of turnip and cheese, prepared as in Series B (sp. gr. 1013), was boiled in an eight-ounce flask for five minutes. Three common test-tubes were superheated and placed in a beaker to support them.

No. 1. The infusion was poured in, and with it one drop of distilled water.

No. 2. The infusion was poured in and thus left.

No. 3. The infusion was poured in and again boiled for two minutes.

These and the flask containing the remaining infusion were left on a shelf for one day; on Dec. 11, there being no cloudiness in any of the four, they were placed on the top of the hot-air bath. On Dec. 13 No. 1 was found to be swarming with *Leptothrix*-growths and free *Bacterium termo*.

No. 2 also was cloudy and swarmed with what Cohn calls the rosary-chains. No. 3 was absolutely free from all development of life, and was perfectly sweet and limpid; so also was the fluid in the original flask, a large one capable of holding eight ounces. How is the development of *Bacteria* in No. 2 to be explained? The original fluid remains pure; the fluid in No. 3, which was reboiled, remains so too; the tube itself, No 2, had been heated red-hot and could not be a source of contamination. One's attention was therefore directed to the conditions of the passage of the fluid from the flask into the tubes; and here an explanation at once offered itself. The large flask *had not been superheated*; its lip was still dirty, laden with *Bacteria* ready to contaminate fluids as they poured from it; hence the contamination of the fluid in test-tube No. 2. The validity of this explanation cannot be disputed, because it is known that such glass surfaces, unless specially cleansed, invariably contaminate infusions exposed to them.

Series G. Feb. 11th.—The publication of Dr. Burdon Sanderson's letter, describing some experiments made by Dr. Bastian, induced us to make a further series of experiments with important modifications. We had expressly avoided the introduction of any thing like visible lumps of solid cheese or turnip into our infusions during their ebullition, believing that such lumps were a possible source of the exclusion of *Bacteria* or their germs from the killing influence of the boiling temperature. This precaution we had supposed (in the absence of any statement to the opposite effect) to have been taken by Dr. Bastian in the experiments adduced by him in the 'Beginnings of Life.' The presence of such lumps was publicly suggested in discussion at the British-Association Meeting at Liverpool as a source of fallacy, and has been demonstrated to be so by Dr. Ferdinand Cohn in experiments made with peas and infusion of peas (‘Beiträge zur Biologie der Pflanzen,’ Breslau, 1872). Further, we had limited the bulk of our infusions and the size of our experimental tubes, in view of the obvious consideration that the larger the mass and area to be guarded against contamination the greater the chance of failure in that respect. Thirdly, it had not occurred to us to make use of vessels in these experiments of a form so inconvenient and difficult to thoroughly guard against effects of "spluttering," and to thoroughly heat by boiling, as the retort. Nor could we guess, in the absence of any directions on that point from Dr. Bastian, that it was desirable to exclude the rind of the turnip from the preparation of the infusion. The correspondence in ‘Nature,’ however, indicated that "pounded" cheese (necessarily in a condition of solid lumps) was added (in some cases) to his *experimental vessels after the turnip-infusion*, and was present during ebullition. It also appeared that retorts capable of holding two ounces were the vessels used; whilst, on grounds not given, it was considered advantageous by Dr. Bastian to peel the turnips before slicing them.

The following experiments were accordingly made:—

An infusion of turnip (minus the rind) was prepared and filtered; it had sp. gr. 1012·7. In the experiments Nos. 34 to 47 two-ounce retorts were used, and the bulb half filled with the experimental infusion.

No. 34. The infusion neutralized with KHO. About two grains of pounded cheese in pellets added to the retort.

Nos. 35, 36. Infusion not neutralized. About two grains of pounded cheese in pellets added to the retort.

Nos. 37, 38, 39. The simple infusion.

No. 40. The simple infusion, to which were added a few drops of an emulsion of cheese prepared with some of the turnip-infusion and new cheese, the emulsion having been filtered.

No. 41. The simple infusion.

Nos. 34 to 40 were boiled for five minutes; they were then preserved in the air-bath at a temperature of 35° C., and sealed approximately during ebullition. Four of them, including No. 36, were subjected to a further boiling of fifteen minutes in a water-bath after sealing.

No. 41 was boiled for five minutes and placed on a shelf with its mouth open.

Subsequent appearances in Retorts Nos. 34–41.

On Feb. 15th Nos. 34, 35, 37 were opened and found to be perfectly sweet and free from a development of *Bacteria* or other organisms.

No. 41 was observed to be perfectly limpid, and is so still (March 17th).

On Feb. 27th Nos. 36, 38, 39, and 40 were opened. With the exception of No. 36, they were perfectly sweet and free from organisms.

No. 36 had a slightly fœtid odour and swarmed with rather long *Bacteria*—that is, *Bacteria* longer than the common *B. termo*, which develops in infusions open to atmospheric air, but not quite of the form of the *Bacillus subtilis* of the butyric fermentation, which is stated to appear in some infusions, *e. g.* milk, to which the access of atmospheric air has been entirely prevented. It is to be noticed that in this series the only retort in which *Bacteria* made their appearance was one of those in which small *lumps* of cheese were present during the subjection of the flask to the process of ebullition and subsequent immersion in boiling water.

This result induced us to make a further series of differential experiments, bearing upon the influence of the state of aggregation of the cheese introduced into the turnip-infusion.

Series H. March 8th.—A turnip-infusion was prepared as in Series B; found after filtration to have sp. gr. 1113·5.

Tubes similar to those used in Series A–E, and half filled, were used.

Tubes 42, 43, 44. The simple infusion was poured into the tube, so as to half fill it; a lump of cheese the size of a pea was then added. Sealed cold.

Tubes 45, 46, 47. To the turnip-infusion, before introduction into the tubes, an emulsion of cheese prepared with turnip-infusion and strained through a piece of cambric was added. The tubes were then half filled with this mixture and sealed cold.

Tubes 48, 49, 50. The same as 42, 43, 44, but sealed approximately during ebullition.

Tubes 51, 52, 53. The same as 45, 46, 47, but sealed approximately during ebullition.

All the tubes, 42 to 53, were completely submerged during five minutes in boiling water, and subsequently preserved in the air-bath at 35° C. temperature.

On March 13th the contents of the twelve tubes were examined with the microscope. No. 45 had been broken in the boiling. The five remaining tubes which had been prepared with cheese in the finely divided condition were found to be entirely devoid of life, the infusion microscopically and otherwise unchanged. Of the six tubes prepared, each with a small lump of cheese, no organisms were detected in 42 and 44; but in 43 and 49 a few elongate *Bacteria* were observed (in the proportion of about two to the field of a Hartnack's system 10). In 48 and 50 the fluid was swarming with elongate *Bacteria* and true *Bacillus*. The lumps of cheese in those tubes in which life appeared had softened and spread out to a certain extent on the side of the tube. The cheese-lumps in Nos. 42 and 44 retained their original form.

From the result of these later experiments, made in consequence of the fuller information given by Dr. Sanderson as to Dr. Bastian's mode of treating turnip and cheese so as to obtain phenomena supposed to be in favour of the doctrine of Archebiosis, we consider that the importance of excluding visible lumps from the experimental infusions is clearly indicated, as also is the comparatively greater trustworthiness of the small tube as opposed to the larger retort for use as an experimental vessel. We moreover consider that we, in our earlier experiments (November and December), carefully following Dr. Bastian's directions, as far as he had given any in the 'Beginnings of Life,' but using at the same time proper care as to cleanliness and due boiling, obtained a series of results contradicting Dr. Bastian's statements as to the spontaneous generation of *Bacteria* in infusion of turnip to which a fragment of cheese had been added.

Further, certain of the experiments above recorded, and others made at the same times with open vessels and simple turnip-infusion, compel us to dissent emphatically from the conclusion of the following statement contained in a recent paper by Dr. Bastian ('Nature,' Feb. 6th, p. 275):—"Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask, with the assurance that it contains no living organisms. Having ascertained also, by our previous experiments with the boiled saline fluids, that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion *invariably* becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*." The italics are our own.

We find not only that such an infusion remains free from *Bacteria* when thus treated (subject, of course, to certain failures in the precautions taken) for "one or two days," but if contamination by the admission of coarse atmospheric particles capable of carrying *Bacteria* be guarded against, it will remain so for weeks and probably for years. In consequence of this absence of development of *Bacteria* we have cultivated *Torulæ* in such a turnip-infusion, so as to obtain them entirely free from the former organisms*.

In conclusion, we would point out that failure in manipulation, contamination in unsuspected ways, such as that due to the preservative influence of lumps, and, again, the mistaking of particles in an infusion which have been there from the first for organisms originated *de novo*, do not exhaust the list of conceivable explanations of phenomena which have been attributed to spontaneous generation. When the knowledge of the natural history of *Bacteria* has advanced somewhat further, there will be a possibility of such explanations presenting themselves in ways at this moment unsuspected.

* At this moment, May 20th, the turnip-infusion in the open retort (No. 41) is free from all organisms, and is perfectly limpid and sweet.

Whilst awaiting Professor Huizinga's fuller account of his experiments, we may point out that the hypothesis of an inhibitory influence of increased density should be supported by experimental evidence, and that it cannot apply to tubes closed before boiling. The neck of the flask closed with asphalt may (so far as conditions are stated by him at present) harbour *Bacteria*, as in our Series F. But especially we would urge upon him and others that it is undesirable, as yet, to introduce into the discussion other organic mixtures. Turnips and cheese may be very bad material for experiment; but it would be well, as far as possible, to settle the matter, or the way in which the matter is to be viewed with regard to them, before going off to other particular cases.

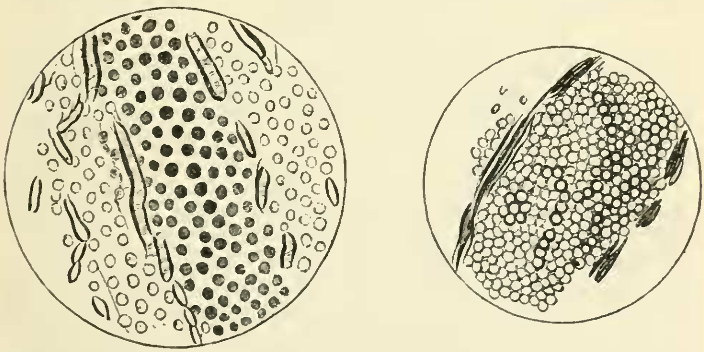
It would be a very excellent thing if all further reference to this subject could be postponed for a year or two—that is, until further study of *Bacteria*, such as that inaugurated by Sanderson and Cohn, has given us surer ground to tread upon.

"Note on High-Power Definition as illustrated by a compressed *Podura*-scale." By E. B. Beaumont, F.R.S., and Dr. Royston-Pigott, F.R.S.

Nothing in microscopic matters has ever afforded us such complete satisfaction as the following result of a very fine definition, accomplished by means of a Gundlach German $\frac{1}{16}$ immersion lens, corrected by a new method, which Dr. Pigott at present delays publishing in the hope of further improvement, but which he is willing to exhibit at his house.

A *Podura*-slide *, fortunately strongly protected with a thick

Podura-scale.



glass cover, having accidentally been subjected to so considerable a pressure as to crush out the structure of a large scale, upon bringing it, by haphazard, into the field of view with a magnifying-power of about 2000 diameters, exhibited a structure indicated by

* *Podura Degeeria* vel *domestica*.

the woodcuts here given, and drawn, in the presence of the writers, at Dr. Pigott's house by the accurate artist Mr. Hollick, deaf and dumb and a rapid delineator without the camera. Mr. Beaumont's surprise and admiration equalled that of Dr. Pigott. This circumstance will excite no surprise when it is stated that for four years the spherules of the *Podura* have been generally denied and warmly disputed. In ordinary cases a crushed scale shows nothing; and as glasses are usually corrected to show the illusory spines or markings, these spherules are concealed.

The idea conveyed was, that two layers of spherules (first detected by Mr. Beaumont within the tubes), like two confined layers of small shot, had, by compression, been forced and largely spread out into broader layers. It was thought also that detached portions resembled long tubes or puckers filled with spherules exactly fitting them. The spherules appeared perfectly spherical, but somewhat unequal in size.

In the general flattened and extended surface of the compressed and disintegrated scale the spherules appeared dark blue or red, according to the slight change in the focal plane, and in a still lower plane white.

In the adjoining uninjured scales long strings of beads were seen, like necklaces of coral, here and there sharply bordered with black lines, apparently denoting tubes of membrane or puckers enclosing them like a tube. Between these strings of spherules peeped forth others of a light orange-colour.

The slide was an old one and well known. The mass of the crushed scale occupied a much broader space than any of the scales.

November 20, 1873.—Sir George Biddell Airy, K.C.B., President, in the Chair.

"Note on the Electrical Phenomena which accompany irritation of the leaf of *Dionæa muscipula*." By J. Burdon Sanderson, M.D., F.R.S., Professor of Practical Physiology in University College.

1. When the opposite ends of a living leaf of *Dionæa* are placed on non-polarizable electrodes in metallic connexion with each other, and a Thomson's reflecting galvanometer of high resistance is introduced into the circuit thus formed, a deflection is observed which indicates the existence of a current from the proximal to the distal end of the leaf. This current I call the *normal leaf-current*. If, instead of the leaf, the leaf-stalk is placed on the electrodes (the leaf remaining united to it) in such a way that the extreme end of the stalk rests on one electrode and a part of the stalk at a certain distance from the leaf on the other, a current is indicated which is opposed to that in the leaf. This I call the *stalk-current*. To demonstrate these two currents, it is not necessary to expose any cut surface to the electrodes.

2. In a leaf with the petiole attached, the strength of the current is determined by the length of the petiole cut off with the leaf, in such a way that the shorter the petiole the greater is the deflection. Thus in a leaf with a petiole an inch long, I observed a deflection of 40. I then cut off half, then half the remainder, and so on. After these successive amputations, the deflections were respectively 50, 65, 90, 120. If in this experiment, instead of completely severing the leaf at each time, it is merely all but divided with a sharp knife, the cut surfaces remaining in accurate apposition, the result is exactly the same as if the severance were complete; no further effect is obtained on separating the parts.

3. *Effect of constant current directed through the petiole on the leaf-current.*—If the leaf is placed on the galvanometer electrodes as before, and the petiole introduced into the circuit of a small Daniell, a commutator being interposed, it is found that on directing the battery-current down the petiole (i. e. *from* the leaf), the normal deflection is increased; on directing the current *towards* the leaf, the deflection is diminished.

4. *Negative variation.*—*a.* If, the leaf being so placed on the electrodes that the normal leaf-current is indicated by a deflection *leftwards*, a fly is allowed to creep into it, it is observed that the moment the fly reaches the interior (so as to touch the sensitive hairs on the upper surface of the lamina), the needle swings to the right, the leaf at the same time closing on the fly.

b. The fly having been caught does not remain quiet in the leaf; each time it moves, the needle again swings to the right, always coming to rest in a position somewhat further to the left than before, and then slowly resuming its previous position.

c. The same series of phenomena present themselves if the sensitive hairs of a still expanded leaf are touched with a camel-hair pencil.

d. If the closed leaf is gently pinched with a pair of forceps with cork points, the effect is the same.

e. If the leaf-stalk is placed on the electrodes, as before, with the leaf attached to it, the deflection of the needle due to the stalk-current is *increased* whenever the leaf is irritated in any of the ways above described.

f. If half the lamina is cut off and the remainder placed on the electrodes, and that part of the concave surface at which the sensitive hairs are situated is touched with a camel-hair pencil, the needle swings to the right as before.

g. If, the open leaf having been placed on the galvanometer electrodes as in *a*, one of the concave surfaces is pierced with a pair of pointed platinum electrodes in connexion with the opposite ends of the secondary coil of a Du Bois-Reymond's induction apparatus, it is observed that each time that the secondary circuit is closed the needle swings to the right, at once resuming its former position in the same manner as after mechanical irritation. No difference in the effect is observable when the direction of the induced currents is reversed. The observation may be repeated any number of times; *but no effect is produced unless an interval of from ten to twenty seconds has elapsed since the preceding irritation.*

h. If the part of the concave surface of the leaf which is nearest the petiole is excited, whether electrically or mechanically, the swing to the right (negative variation) is always preceded by a momentary jerk of the needle to the left, *i. e.* in the direction of the deflection due to the normal leaf-current; if any other part of the concave surface is irritated, this does not take place.

i. Whether the leaf is excited mechanically or electrically, an interval of from a quarter to a third of a second intervenes between the act of irritation and the negative variation.

MISCELLANEOUS.

Observations on the Existence of certain Relations between the Mode of Coloration of Birds and their Geographical Distribution. By M. A. Milne-Edwards.

In carrying on my researches on the geographical distribution of animals in southern regions, I have been struck with certain relations which seem to exist between the parts of the globe inhabited by birds and the mode of coloration of those animals; and wishing to know the degree of importance that ought to be ascribed to this observation, I have tried to examine more carefully than had previously been done what may be called the geographical distribution of colours in birds. In fact this investigation seemed to me capable of throwing some light on the influence which local biological conditions may exert upon the secondary zoological characters of species and races. To furnish significant results it ought to bear principally upon the natural groups which have a very wide geographical distribution; and in order that it may have the necessary degree of precision, it ought to be founded upon the chromatic analysis of the plumage and the comparison of its colours with well-defined normals. Without the aid furnished by the chromatic circles, for which science and the arts are indebted to M. Chevreul, it would have been difficult for me to appreciate thoroughly the tones and shades which I had to take into account, and still more difficult to formulate clearly the results furnished by observation; but by means of these circles this labour has been remarkably facilitated.

In a first series of investigations I attended specially to various degrees of melanism; and in order to judge of the relative influence of black upon the plumage of birds inhabiting various geographical regions, I took into account not only the extent of the parts of the tegumentary system which are tinted in that manner, but also the degree in which the other colours may be dulled or modified in their tone by mixture with black in various proportions.